

THE
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GENERAL REVIEWS AND SUMMARIES

CORRELATION

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Simpson (17), working under Thorndike's guidance, has made the most extended contribution during the year to the problem of the correlation of mental abilities with each other and with "general intelligence." He applied fifteen tests to two groups of men of extremely different types of ability, testing each man individually. The good group consisted of seventeen professors and advanced students of Columbia University, the poor group of twenty men who "had never held any position demanding a high grade of intelligence"; eleven were staying at the Salvation Army Industrial Home until they could get work, seven were in a mission on the Bowery.

In the interpretation of his results Simpson finds justification for the assumption of "general intelligence," but not for Spearman's common central factor. "On the other hand certain capacities are relatively specialized, and do not necessarily imply other abilities except to a very limited extent." Subject to the limitations of the experiment he believes that "general intelligence" implies the different abilities tested in the relative order (a) selective thinking, (b) memory and association, (c) quickness and accuracy of perception, (d) motor control, (e) sensory discrimination. This conclusion rests in part upon the closeness of relation of each of the tests for these capacities with the others. It is corroborated by the kind of tests in which the poor group most overlaps the good group. With a combined score of five of the more distinguishing tests he finds that not one of the poor group reaches the lowest of the good

group. As confirmation of "the view that by far the most influential factor making for efficiency in these tests is the native capacity of the individual in question, and not simply his training and environment," he cites the low correlation (.38) between rank in eight of the best tests and number of years schooling.

As a matter of method Simpson compares the correlations within each of these extremely different groups and with the combined group, thus using these three coefficients for the same test to check each other. In the author's opinion this gives "a far more accurate idea of the true amount of the correlations among abilities for people in general." Presumably he means a better idea than would be obtained by testing the same number of unselected individuals. Although he recognizes that spurious correlation results from combining two extreme groups he apparently thinks that he has checked this by comparing the coefficients of the combined group with those within each of the extreme groups.

From his table of "estimated true coefficients," prepared by averaging the coefficients for the poor and good groups and then averaging that result with the coefficients of the combined group, he calculates the average of the correlations between each of twelve tests and the rest. In order these are as follows: hard opposites .60, Ebbinghaus .58, memory of words .56, easy opposites .53, A test .50, completing words .47, memory of passages .44, adding .43, learning pairs .41, recognizing geometrical forms .40, estimating lengths .26, drawing lengths .13.

The correlation between the combined score for five of the more distinctive tests with the estimates of general intelligence within the good group he finds to be .92, after correction. The reliability of the combined score in this group is .72, as tested by the first and second trials. The opposites, Ebbinghaus, and memory of words show the closest relation to estimated intelligence, two giving coefficients of over .90.

The product-moment formula was used for calculating the coefficients, the central tendencies were measured by the medians, and reliability coefficients were determined for each test. The coefficients are corrected for chance observational errors by Spearman's formula, but Simpson feels that the extra labor involved might better be spent in making more accurate measures of each individual. Instead of being "attenuated" by errors of observation, a number of the raw coefficients seem to have been "fatter" than after correction. He proposes to test the Spearman hypothetical ex-

planation of correlation through the relationship to one common central factor by showing that two tests correlate more closely with each other than the common element in them correlates with the common element in two other tests. This he supposes would be impossible if all correlation was explained by the Spearman hypothesis. Neither does he find that his coefficients take the form of the Spearman hierarchy. An important critical résumé of the work that has been done in the investigation of the relationship of mental abilities is included in the monograph.

The close relation between mental deficiency and the extreme form of alcoholism which goes with admission to the Inebriate Reformatories is confirmed by Heron's study (7) of 865 female inmates from the data collected by the inspector of the British reformatories, Dr. Branthwaite. Not only are the conclusions of great importance for the scientific explanation of alcoholism through heredity, but the selection of different equations for correlating various kinds of data forms a model for similar studies. Correlations are determined for sixteen different characteristics bearing upon alcoholism, and either r or η is given for each relationship. The mental condition was determined by the estimation of the Inspector, whose opinion thus remains as constant a quantity as is possible for such estimations. He classified 554 as mentally defective. The author concludes that "we are on fairly safe ground in asserting that the relationship between inebriety and mental defect is about .76." Furthermore there is a close relationship between mental defect and education, which is "largely determined by causes which are pre-alcoholic." "This is strongly in favor of the view that the defective mental condition of these inebriates, like the extent of their education, is pre-alcoholic and that the alcoholism flows from a preëxisting mental defect, not the mental defect from the alcoholism." "The amount of mental defect among those who have been drinking for many years is only slightly greater than that among those who are at the beginning of their alcoholic careers. There is a close relationship between the intensity of alcoholism and the mental condition of the inebriates but no relation with their physical condition." The latter is judged by the ability to do hard work in the reformatories and this lack of relation to intensity of alcoholism is brought out when allowance is made by partial correlation for constant age at onset and constant duration of alcoholism. "All this lends support to the view that the mental defect of the inebriate is not a gradual growth; it is born, not bred; that inebriety is more an incident in the life of the inebriate than the cause of his mental defect."

As indicative of the importance of the partial correlation method Heron shows clearly that Branthwaite is mistaken in his conclusion that his figures "demonstrate the influence of continued drunkenness and the life associated with drunkenness in the making of the unfit." The figures indicate a definite and regular increase in the percentage of those physically unfit for work with every additional decade of habitual drunkenness; but the partial coefficient, with age constant, is only .04. This proves that the relation between duration of drunkenness and unfitness is only the indirect effect of increase in age.

The attempt to determine the relation of correctness of memory to a number of factors, in particular to volitional attitudes of truth-telling, has brought Franken (5) to use the method of correlation for obtaining a general picture of the situation. His new plan of decision and precision questions (*Entscheidungs- und Bestimmungsfragen*) which cannot be taken up here, provides that the subjects first commit themselves as to whether they know certain facts and then be tested by questions as to the correctness of their decision. Among the various correlations which he studied the following are some of the more important: Those who give many correct decisions also, as a rule, give many incorrect decisions, although few untruthful ones if the untruthfulness is judged relative to their opportunity for incorrect report. Those who permit themselves to give many untruthful answers also make many incorrect decisions. The formal characteristics of memory span, readiness, correctness, show positive correlations with each other. For example, the corrected coefficient for rich fund of knowledge with ready control of it was .439, which he thinks is contrary to popular opinion. The decisions of those with average knowledge possess usually the smallest truth value. Those with a limited extent of knowledge can thank their lack of ambition for the high truth value of their decisions. Those with ability oppose their better judgment to the opportunity for untruthful decisions. The well-informed are as a rule ambitious but also more truthful than the less informed. The more readily a person asserts the correctness of his knowledge the less likely is he to have much knowledge. The conclusions are all limited to the conditions of the experiments, which were carried on mainly with pupils in school and bore upon their knowledge of geography, history, biography and literature.

Harris (6) collects the data as to the correlations of husband and wife in various characteristics including stature, span, forearm,

cephalic index, hair and eye color, general health, duration of life, intelligence, temper, temperament, success in life, nervous and mental disease, economic position. The amount of data when thus brought together is quite surprising and supports his contention that it is "highly probable that a great variety of physical and mental characters influence human matings, and in such a way that, on the average, similar individuals tend to marry." Pearson and Elderton (11) calculate that the correlation between parents and children in general health is about .38, somewhat less than that for physical characteristics, which for these classes is on the average .46. The previous work of Macdonald and others suggesting that dark types are less likely to disease is not confirmed by the investigation of Saunders (15) of the relation of pigmentation to infectious diseases among Birmingham school children. The disagreement with Macdonald may possibly be explained by the differently composed populations of Birmingham and Glasgow. The results confirm Elderton as to no correlation between pigmentation and height or weight.

Terman and Hocking (22) after showing practically no correlation between hours of sleep and school standings in any subject, nervous traits, or social status, for every age from 6 to 16, with 2,692 pupils, contend that this does not prove that any person should not sleep as much as he can, since the factor of safety which requires an excess of sleep over immediate needs is unknown. Winch (25) finds that a group test for visual memory of letters given to a class of 45 thirteen-year-old boys showed reliability coefficients from .87-.90 for the sixth, seventh and eighth trials. For a group averaging eleven years a simpler test of auditory memory for letters showed coefficients of about .74 for the first four trials. Moreover he found that on subsequently dividing these groups into two portions and testing the parts under different conditions of fatigue the individuals in the two halves who held the corresponding ranks retained their same relative positions in the tests under new conditions with a correlation of about .86. In a more recent paper (26) he gives similar results with the use of arithmetical tests on groups of school children in investigating mental adaptation.

Pyle (13) shows the interrelations of a number of group-tests for mental activities given to a group of several hundred adults for the purpose of selecting tests to be used on large groups of children. He finds that Whipple's marble statue test for logical memory showed the highest average correlation with the other tests used.

Immediate memory in this test correlated with retention after five weeks by .76 and .70 in two different groups. Although this immediate memory test showed the highest correlation on the average with the other tests, that average was only .29. With some school grades it correlated .69 with class standing. Four tests used by Burt are repeated on 115 college freshmen at the University of Texas by Calfee (2) and she finds much reduced coefficients in the correlations of the test with each other and with class standing when she gives each test only once, compared with Burt's results when he took the average of two applications of the tests. Hoyt's study of the relation of knowledge of grammar to the ability to write compositions or interpret poetry into prose is repeated in part by Rapeer (14) who verifies Hoyt's low correlations, the highest coefficient being .24, with 200 high school students.

An important paper is contributed by Pearson (9) dealing with the best methods available for the correlation of variates classed in a few broad categories. It provides a number of formulæ for making corrections when the material is classed in an $n \times n$ -fold table. Besides considering the correction for the use of class indices, the paper takes up the correlation between the true quantitative value of a variate in any individual and that individual's class mark. In another paper Pearson (10) gives a short method for calculating the error of r when found from a four-fold table. Everitt (4) gives supplementary tables for facilitating the determination of the correlation coefficient by the method of the four-fold table in cases where the correlation has a very high value. In (21) we have a correction to be made to the correlation ratio for grouping under special condition.

Three of Yule's formulæ, his "coefficient of association" (Q), his coefficient of colligation, and his correlation-coefficient for a two \times two-fold table are emphatically attacked in an extended paper by Pearson and Heron (12). They give a complete theoretical discussion of the assumptions on which the formulæ are developed. "The third of these coefficients they term a method of pseudo-ranks. 'The coefficients of association and colligation are in our opinion wholly fallacious, they represent not true properties of the actual distributions, and they have no adequate physical interpretation. The coefficient obtained by the method of pseudo-ranks is equally fallacious, unless the variables proceed by and have been grouped by discreet units.'" The authors regard these formulæ especially dangerous to statistical work because they are so easily applied that

already they have been used by a number of investigators. The values of Q and Pearson's tetrachoric r are wholly different and tend in opposite directions as the point of division of the attributes is changed. "The new 'coefficient of colligation' is really an old friend, which under the form of Q (sub-3) did not possess the 'fundamentally different properties' with which Mr. Yule credits it." The writers contend that the controversy between Mr. Yule and them is "the old controversy between nominalism and realism," since they claim that Mr. Yule is using class-names which represent discrete units as if they were real units, when in reality the attributes thus treated are continuous variables and should not be treated as if discrete. Yule's simple proof of Sheppard's correction for the influence of grouping on the standard deviation is demonstrated by Pearson (8) to be fallacious.

Important labor-saving formulæ are provided by Spearman (19) to be used after the correlations of several series of values have been calculated, when it is desired to determine the correlations of some of the values averaged together, added or subtracted. The formulæ supplement that given by Woodworth for combining results of several tests, and enable the investigator to make allowance for the differences in the standard deviations of the series without the labor of reducing each measurement to its proportion of the deviation. The equations are expected to be serviceable where the experimenter, for example, wishes to discover which form of pooling tests will give the closest correlation with estimates of "general intelligence." Moreover the formulæ show that the customary replacement of the correlation of averages by the average of correlations cannot under any circumstances lead to the right result. Spearman finds that several equations given previously by him are corroborated by the present demonstration and that they are correlaries of the more simply obtained new formulæ. This applies to the equation for increase of correlation obtained by increasing the number of measurement. Soper (18) develops an equation for determining a mean value of correlation coefficients obtained from small samples and the standard deviation of these coefficients from this mean value. When the coefficient calculated from a small sample is small, the defect in it he estimates may be of the order of 5 per cent. in samples of 10 and of .5 per cent. in samples of 100. He gives some experimental confirmation for his method of calculation. Pearson (9) gives the proofs that the formulæ showing the general influence of selection on correlation and variation are independent of any Gaussian

assumption. Wagner (24) provides a formula for giving a mathematical expression to the direct relation between a series of measurements and their repetition. Spearman (20) and Betz (1) continue the discussion of the value of Spearman's formula for correcting the correlation coefficient for chance observational errors. Spearman explains that it is the practice of his laboratory when the correction of the coefficient is over 10 per cent. to take further measurements, if possible, or otherwise to improve the method of measurement. He contends that in any case, however, the corrected coefficient is better, and that without it the customary comparison of coefficients is illusory. Betz replies that Spearman thus admits that when the correction is large it is of doubtful efficiency.

In the revised edition of his *Mental and Social Measurements*, Thorndike (23) has elaborated his chapter on The Measurement of Relationships into two chapters. In one chapter he repeats his discussion, in somewhat more elaborate form, of the effect of uncertain zero points in calculating relationships and takes up in addition the variety of methods for expressing this relationship. He does not, however, in either chapter set forth Pearson's method for calculating the correlation ratio when the relationship is not rectilinear, or for testing the question whether the relationship is rectilinear. He apparently prefers, for his simplified treatment, to depend upon the presentation of all the facts in a correlation table and then calculate the relationships desired. In conformity with one of the purposes of the new edition to amplify the discussion of what the various procedures measure, he devotes the second chapter to a more elaborate treatment of the problem of correlation, the data available for estimating it, the methods of calculating it, the cautions to be observed in interpreting the results. The translation into English of Schulze's *Experimental Psychology and Pedagogy* (16) may make a word of caution necessary in regard to his chapter on Psychical Correlations. The method of applying the product-moment formula directly to variations of ranks from the mean rank, which is given by an example in detail, would be generally condemned at present. The chapter attempts to suggest the importance of the correlation method to pedagogy.

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DYNAMIC PSYCHOLOGY

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Quite the most conspicuous contribution is that of Watson (8). It is an unusually concrete statement of a central idea that has always claimed certain adherents among us, at least as an idea. Therefore the way in which many have received it seems to be due not so much to either its source or content as to a changed attitude in those who read its words. A well-aimed blow at the autistic method in psychology, every one who was at Middletown knows that it came home. Among its nuclear sentences are:¹ "The psychology which I should attempt to build up would take as a starting point first, the observable fact that organisms . . . do adjust themselves to their environment by means of hereditary and habit equipments. These adjustments may be very adequate, or they may be so inadequate that the organism barely maintains its existence. . . . Psychology has failed signally, I believe, during the fifty-odd years of its existence to make its place in the world as an undisputed natural science . . . as it is generally thought of, has something esoteric in its methods. . . . For a 'pure' psychologist to say that he is not interested in the questions . . . because they relate indirectly to the application of psychology shows in the first place, that he fails to understand the scientific aim in such problems, and secondly that he is not interested in a psychology which concerns itself with human life." The first of these is not improbably the product of the second. "One of the earliest conditions which made me dissatisfied with psychology was the feeling that there was no realm of application for the principles which were being worked out in content terms. . . . Should the human psychologists . . . refuse to modify their position, the behaviorists will be driven to using human beings as subjects and to employ methods of investigation which are exactly comparable to those now employed in animal work." Well, there are many who have had Watson's difficulties, if not everyone would have given them so judicious—or moderate—expression.

Although delivered before Watson's paper, that of Angell (1) is

¹ Not quoted in the order in which they occur.

published subsequent to it and should also be read after the other. The rôle properly assignable to introspection, which in Watson's paper centered about the "language method in behavior," was left in need of a clearer formulation, and this is what Angell's paper is especially valuable for. He takes the position that if one restricts himself uncompromisingly to the objective means now at hand for the interpretation of behavior one obtains an account not only very incomplete in itself, but with very little prospect of ever being completed by objective means. There is no doubt of a real loss here, and while Angell does suggest a line of defense on the ground of its unimportance, it is only with a "perhaps" that looms very large on the mental horizon of a psychopathologist. Only a behaviorist who is prepared to give up all the "pragmatic sanction" of behaviorism can be expected to dispense with the data of those mental reactions of which no account is obtainable save in linguistic descriptions. It ought to be very clear that objections to introspection adduced by the psychoanalytic school are strictly objections to *rationalizing* from introspection. Technically, the psychoanalytic method is itself introspection raised to the n th power, so that it would be surprising if members of this school should see fundamental difficulties in the way of its descriptive functions. Such would more probably be that introspective data on any significant matter, unless obtained under psychoanalytic conditions, were too apt to be falsified.

Last, not least, Angell calls attention, objectively, to the temperamental factor in psychological attitudes, a topic that would repay much further development. Let it be repeated that the two papers supplement one another admirably, and should by all means be studied together.

In a brief note Miss Calkins (4) "admits and indeed emphasizes" the indictment on the grounds of remoteness from life, but makes a point of defending introspection as a mental discipline. Attention is again called to the ineffectiveness of the "objective" method when similar reactions ensue to different stimuli, or opposed reactions with very allied mental states. The intellectual position seems quite close to that of Angell.

Preëminent on the constructive side are the formulations by Bleuler (2) of the concept of autistic thinking, and by Dodge (5) of the theory and practice of measuring mental work. While neither is one of those who must needs make things easy for the reviewer, Bleuler has given the main points of his conception in a summary on which there would be little purpose in trying to improve. It runs, substantially,

There is a kind of thinking, which is independent of logical (*resp. experiential?*) laws, being governed instead by affective motives, autistic thinking.

It is seen most prominently in dementia præcox and in the dream, also in mythology and in superstition, in the day dreams of the hysterical and normal, and in poesy.

Autistic thinking is able to serve its purposes with wholly unlogical material; sound associations, chance coincidence of perceptions and images may take the place of logical associations. Concepts partially thought out, false identifications, condensations, displacements, symbols with the value of realities and similar abnormal psychisms make up a part of the material which is employed in autistic thinking. Obviously, however, normal material and normal thought processes are by no means disregarded.

Logical thinking, conditioned by reality, is a psychic reproduction of such combinations as reality presents to us.

Autistic thinking is governed by the instinctive trends; it follows the direction of the trends without reference to logic and reality. According to the familiar laws, the affects underlying the instinctive trends facilitate associations corresponding with them, and inhibit contradictory ones.

It is a part of our tendencies to withdraw from unpleasant experiences not only coming from without but occasioned by mere images. The immediate result of autistic thinking is therefore the creation of pleasant images and the suppression of unpleasant ones. *The primary function of autism is wish-fulfilment.*

But where a negative mental attitude is present, the trend of autistic thinking may be negative. This is the case on the one hand in melancholic depressions, on the other hand when the autistic imagery is brought into too sharp conflict with reality.

In the melancholic depressions, autism is expressed in depressive delusions, distinguished from those of the ordinary depressions in their greater detachment from reality.

The unpleasant feeling of the conflict of autistic ideation with reality leads to delusions of persecution.

Autistic thinking may be conscious or unconscious in the same way as logical thinking. But in dementia præcox its finished products are apt to appear as hallucinations, primary delusions and falsifications of memory whose elaboration has taken place in the unconscious.

There may also be a variety of autistic thinking which rather satisfies logical necessities in unlogical ways (for example certain portions of mythology and symbolism), and in which the affective motive is secondary.

Autistic thinking is not a primitive form of thought; it could develop only after thinking in memory images considerably preponderated over the immediate mental reaction to real, external situations.

Ordinary, realistic thinking, the "*fonction du réel*," is fundamental, and to a viable organism with a mind, is as essential as reaction in accordance with reality.

That the weakening of realistic thinking brings about a preponderance of the autistic is obvious, because realistic thinking with memory images must be learned through experience while the autistic proceeds according to mechanisms that are inborn. These may make use of any imaginal material according to principles inherent in every organism.

That the rôle of autistic thinking is so great, and is not eliminated by selection, is probably due on the one hand to the fact that it is impossible for a finite intellect to define the boundary of realistic and autistic thinking, and on the other hand that

pure autism has its value as mental discipline,¹ just as physical play on the bodily side.

Nevertheless, its phylogenetic significance is still in many ways unclear to us, for example in its extension to art.

It requires some familiarity with the underlying mechanisms of personality, in which pathological experience is especially helpful, to grasp the full meaning of these passages; only a slight earnest of the observational data on which they are founded can be given in the body of his paper.

Dodge approaches our problems from an entirely different angle, a definite experimental inquiry calling for highest refinements of technique, and is one to whom such problems are not likely to call in vain. The earlier portion of his article is distinguished for an exceptionally frank statement of the difficulties involved with respect to incentive, interest and impulse, together with what he believes to be a tendency to greatly misconstrue the affective values of different portions of the experimental situations (pp. 16-19). The criterion taken for mental work is a metabolic one, given in the pulse rate, but recorded, perforce, under very different conditions from those which prevail in ordinary sphygmographic measurements. The present sphygmograph operates electrically, and is a telephone receiver actuated by the temporal artery, recording upon a string galvanometer, thus providing for the subject freedom of all ordinary movement, and the essential separation from the registering apparatus. Experimental data are given chiefly by way of illustration, showing the effects of small mental operations, and of the work incidental to examination periods. In this way different sorts of work may be roughly equated according to their having equal or similar metabolic resultants. Thus the mental processes of multiplication approximate somewhat to the raising of a one-pound weight every four seconds. To arrive at an adequate estimate of the mental work of examination it becomes necessary to control the writing movements and also certain gross bodily activities, both efficiently accomplished by means whose ingenious simplicity is characteristic. The indication of the records quoted is in Dodge's own words: "the muscular demands on metabolism during the examination period incident to the process of writing are conspicuously less than the demands of the distinctly mental processes." The highest pulse rate tends to be found in the first non-writing period, when the questions are first seen, "the period of most active mental adjustment." There is generally a gradual fall to the end of the exami-

¹ *Vide* Calkins, *supra*.

nation period. The paper concludes with some theoretical formulations to fit these and other observations; the whole marking a decided step in the development of objective method.

The study by Haggerty and Kempf (6) is significant as the first successful attempt in an important field which has hitherto been approached experimentally only by the free association method. Comparing groups of men and women in the controlled association tests arranged by Woodworth and Wells, they found an average of better performance by the women in cancellation, naming, substitution and directions tests, but an average of poorer performance in the large group of tests included under the head of "logical relations." A number of general possibilities are unfavorably considered by way of interpretation, among which it may be mentioned that the situation regarding the personality of the experimenter was not such as to make this a probable factor in the differences observed. The actual interpretation seems to be along the following lines: the tests of "Group I." make few demands on *choice* in the subjects, though they may make considerable ones on *rationcination*, as in the directions tests. The *logical relations* tests, on the other hand, make much greater demands on choice, though less than the free association experiments. Now it is to be noted that certain periods of confusion, producing lengthened reaction times, are present in this type of controlled association reactions quite as truly as in free association reactions, and are indeed often more clearly defined here. Examining the results in this direction, it is found that these episodes of "confusion" are about twice as frequent in the women as in the men, and it is of this that their lengthened time in this type of test is the expression. Conflict, suppression and substitution are the verbal forms which the authors give to their ideas of the causes of these episodes, and while they do not mention the term it is quite probable that they have the psychoanalytic work in mind. The results are, indeed, quite confirmatory of what may be observed with the free association experiment, and presumably have the same origin. The presentation does not actually bring out how consistent the group difference is or how much the two groups intergrade. But since the authors raise the issue of "importance" in respect to the main fact, it should be brought out that it is not as a sex difference that it is important, but as a tempermental one between individuals of whatever sex. We are in little need of new criteria for sex; we are in great need of experimental correlations with mental habits and personality. As the study helps to point the way in this direction it is a genuine contribution to dynamic psychology.

Karpas (7) presents a conception of the constitutional psychopathies formulated in terms of mental adjustment. He describes concretely the way in which various pathological reactions may manifest a disproportion of affect, intellect and volition. The unbalanced factor may lie in any of three spheres and produces a definite type of personality accordingly. On the emotional side he distinguishes two groups of subnormal affective life, the regular shut-in personality and a "seclusive personality subject to tantrums." In the hyperaffective group he includes the manic and a hysterical temperament without clearly defined conversions or complex-fixations. These individuals, and the second group of seclusive personalities above mentioned, present a feature of special psychological interest in their subjection to transitory psychotic flurries, on the one hand of the tantrum, on the other often of a panicky nature, whose mechanisms, when they can be observed at close enough range, are quite instructive in the matter of mental hygiene. While these varieties tend rather to personal difficulties, it is the lack of volitional balance which most frequently leads to conflict with society through the medium of the law. Various criminal and poorly controlled reaction types are described, but their definitions seem not so clear in the author's mind as with the previous groups. The intellectual forms of defect are dismissed quite briefly, perhaps because they are otherwise the more familiar. There follow some general remarks under the headings of diagnosis, prognosis and treatment, concluding with a summary of the Binet-Simon tests. The chief value of the paper lies in its clear emphasis upon the "inferiority" as a maladjustment, and the amelioration of this adjustment as the chief therapeutic point of attack.

Burrow (3) contributes a thoughtful paper on the consequences of a far-reaching penetration of the present psychoanalytic doctrines into the social mind. In the first instance he points out the necessary subversion of religious beliefs, and in the genetic analysis of those groups of mental activities associated with the intensest feelings, the destruction of those "springs of primitive sentiment which have actuated all that is best in human conduct." He mentions how these interpretations are applied to a number of specifically biblical teachings, and voices the "unhappy" conclusion that philosophically psychoanalysis becomes a name for the "utter abrogation of religion and the apotheosis of sex."

The longer portion of the paper is concerned with the more definite issue of *Ausleben oder Neurose*. The neuroses are of course

held to be conditioned by failure of sexual adaptation, but Burrow seems to view the issue rather more simply than it really is in the Freudian mind. He does not introduce the distinction between actual and psychoneuroses nor the for him most important one of neurasthenia and fear-neurosis, but takes up only the possibility of neurosis brought about by suppressed sexual activity, and meets it with the readily to be admitted fact that neuroses may be present in those without noticeable somatic repressions, indeed among "veritable Don Juans." The difficulty is mental, as Freud himself pointed out.¹ But even though *Ausleben* were to provide a solution, it is not one which Burrow would countenance. Psychoanalysis must recognize its responsibilities to society as well as to the individual and must not give the sanction of "cruel nervous disease" to conduct subversive of social order. "*Und wenn Sie auch Protestantisch sind, deswegen dürfen Sie doch kein Meineid schwören!*"

It is difficult to have many fears about the broader issue. Newton's discoveries did not disturb the properties of gravitation, but furthered their more intelligent use; and nothing worse need be expected of the factual material of psychoanalysis. The most immediate hope of its justification to society lies in its furthering with all of us the last step of what Disraeli said were the three tasks in the career of the English lawyer: to get on, honor,—and honest.

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¹ Freud, S., *Selected Papers on Hysteria and other Psychoneuroses*, pp. 184-185. Cf. also *Amer. J. of Psychol.*, 21, 1910, 217-218, and Jones, E., *Psychoanalysis*, Chs. XIX. and XX.

VOLITION AND MOTOR CONSCIOUSNESS

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Ribot believes that every state of consciousness is a complex whose stable, resistant portion, its skeleton, is formed by motor elements, kinæsthetic sensations. In (5) he shows this, confessedly as a matter of probability, of simplicity and adequacy of hypothesis, rather than of full proof, for the intellectual states. These all contain kinæsthetic elements more stable than the accompanying elements from the special senses, though the latter may obscure the former, and yet the former form the synthesis of the latter, their support and principle of permanence. All association is an association of movements, and in its mediate form the mediating member remains unconscious and is constituted solely by motor elements. Attitudes also are forms of motor activity, with a very feeble coefficient of consciousness. Motor activity interpenetrates and envelops all our psychic life, and forms its solid portion. Under the form of presentations or representations—percepts, images, concepts—it contributes to the formation of all states of consciousness and of their associations, and it constitutes those dispositions, general and momentary, called attitudes. In (6) he shows that motor images possess the same indispensable character in the affective states, and that they form that portion of the psychic field commonly called unconscious or subconscious. In regard to this latter, one class of psychologists holds that the phenomena are genuinely, though faintly and imperceptibly, conscious; another class, that they are psychically non-existent. In opposition to both, Ribot contends that what exists in this field is the kinæsthetic portion of states of consciousness, motor phenomena that more than all others possess a tendency to organize and solidify themselves.

Schumann's book (7) is here merely cited for reference, without having been examined by the reviewer. Martin (4) writes of the psychology of volition with an aim directed particularly toward its pedagogical applications. Consequently he is less concerned with the full analysis and inner mechanism of the phenomena, than with their general forms and sequences. The general character of the book can be described best in the words of Malapert, who contributes the preface: "What interests him most is the development of voluntary activity, the different forms that it may take, which appear to

him as successive phases through which in the course of its evolution the will normally tends to approach toward the finished, the typical volition; and then the rôle played in this evolution by the intellectual functions, the impulses, and the feelings; and further, the many conditions and influences that can further or hinder the formation of this completed will, whose characteristics are unity, stability and morality. Finally the work indicates the principal means that the educator can use for strengthening, organizing, clarifying and moralizing the will of the child, for teaching it the technique of willing, whereby it may facilitate the intervention and triumph of its will and know how to deliberate, decide and execute rationally."

Dauriac's article (1) is a review of experimental work by Mechatte and Prüm (*Le choix volontaire*, *Arch. de Psychol.*, 1910) and by Barrett (*Motive Force and Motivation Tracks*, 1911). He describes their experiments, and defends them as genuine observations of the process of deliberating, choosing and acting, instead of being, as might be assumed, mere matters of suggestion, of submission to the prescriptions of the experimenter.

The problem of freedom is touched upon by both Martin (4) and Dauriac (1). The former gives it very little discussion. His attitude seems best summed up in the statement in his introduction (p. 3): "The will is free. . . . This is a power marvelous and incomprehensible." Dauriac holds that whether or not man is metaphysically free, yet psychologically there is no doubt as to the fact of free voluntary activity. I can resist a passion, can strive against a given class of causes and reduce their action to an "influence," and am so little slave of my ideas that in absence of every favorable motive, I can deliberate and cause just this motive to appear. Therein I am free. But this psychological freedom is not necessarily in conflict with determinism. A triumph over passion is a triumph, even if fated.

The two remaining articles deal specifically with the question of freedom and determinism. Foltz (2) argues for "automatism." The appearance of indeterministic action is due to the inadequacy of perception to unravel its complexities, and the resulting ignorance of the causes that are at work; but this ignorance is no evidence of the absence of deterministically working causes. Suffering is due to incomplete organization; and it is beneficial, banishing inertia and leading to better specialization and organization, the essential features in progress. Rightly understood, determinism does not discourage effort, nor produce a feeling of helplessness and irre-

sponsibility, of "weight and pressure of the rule of mechanism" in those who believe in it. "I can see no reason why we should not welcome with open arms a conception so beneficial to the body, to the understanding and to the craving of the heart."

Gaultier (3) claims that the idea of liberty or free-will is a concept the most contradictory, the most shadowy, the most devoid of meaning that the human mind has ever devised. It is impossible to find place for it in a philosophy of reality as it presents itself; and it is irreconcilable with the postulates of the philosophy which has devised it. To be sure, as the believers in liberty wish to establish, there is something else at work in the world besides determinism; but this something else is altogether other than what they conceive as freedom.

Phenomenal existence springs from a double source. Its principle of determinism is a matter of constant relations, and gives rise to science, calculability, prevision. But the questions, why one phenomenon leads to a definite other, why any substance has any particular properties, why force is attached to one character rather than another, can be answered only in terms of an improvisation—an arbitrary factor, an "alea," a fatality—whereby life begets itself according to the multiplicity of its modes, independently of all constraint and of all reason; and this improvisation introduces into the phenomena of existence an irreducible feature of the incalculable, of ignorance, of uncertainty, of contingency. Some eliminate this fact of improvisation, and wrongly make the world wholly deterministic. But the libertarians attempt to introduce between the factors of improvisation and of determinism a new factor, liberty, thus denaturizing improvisation and conferring on it properties that it does not possess.

The idea of liberty is a delirious invention of sick minds, dissatisfied with existence as it is given. Finding suffering, they invent fault to account for it. Inventing also justice, they invent free-will to justify punishment for fault. These are all fictions, necessitating one the other, and all of them unfounded and contradictory.

The moralists have believed that if they could reserve some domain of life from the inflexible play of causality, this would prove the existence of liberty. Their opponents have singularly accepted the dialectic strife on this ground. Instead of denying their premise, the author strengthens it by showing that causal necessity is a case of contingency; that the "unforeseeable" which Bergson, and the "contingent" which Boutroux thought were identical with free-will,

are not such at all, but are cases of the incalculable and the arbitrary involved in his idea of improvisation, which supplements determinism but never introduces itself into its field as an exception to its applicability.

There is an absolute contradiction between the terms choice and freedom. A choice is a choice only on condition of being strictly determined by an inclination, immediate or reflective. By definition, contingency is something that we cannot foresee, create, favor or resist. A person could never be held responsible for a leap of the will that a sudden irruption of contingency (if contingency took this form) had produced in him. We can free the will from determinism only by subjecting it to a rigorous form of fatality. However we take reality, as it is, or with any manner of distortion, and in whatever manner we try to formulate free-will, it is a conception full of contradiction, devoid of genuine meaning, and powerless to support the ideas of responsibility, of merit and demerit, of the justification of punishment and reward.

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FATIGUE, WORK, AND INHIBITION

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Sherrington¹ has shown that "when the scratch-reflex elicited from a spot of skin is fatigued, the fatigue holds for that spot, but does not implicate the reflex as obtained from the surrounding skin." Forbes (6) reports that stimulation of one reflex arc until it is fatigued does not affect another allied arc. He, consequently, declares that the fatigue does not involve the motor neurones but

¹ C. S. Sherrington, *Integrative Action of the Nervous System*, 1906, 218.

that its seat is in the synapse. That is, the reflex center is not fatigued as a whole, but merely the particular channel of approach which has been employed. From a study of the refractory phase of the wink reflex Dodge (5) concludes that mental fatigue phenomena do not operate to prevent absolutely the repetition of an act or to increase necessarily the time of performance, but rather bring about a tendency toward not repeating the act. Successive repetitions thus require a constantly increasing stimulus or else the interval of time between each will be successively lengthened.

Arai (1) carried on an extensive series of tests in mental multiplication on herself for periods extending up to 12 hours in length. Fatigue was shown by a gradual increase in the length of time taken to do the examples,—an increase which amounted to about 100 per cent. after 12 hours of continued work. Similar tests on a group of subjects for 2 hours showed an increase of 24 per cent. in the time taken to do the examples. Arai also reports (1) that “there is a decrease of pulse rate as a result of continuous mental work,”—a decrease which is positively, though slightly, correlated with decrease in efficiency of the work; (2) that “the feeling of fatigue is somewhat, though far from perfectly, correlated with the state of mental inefficiency”; (3) that “fatigue in a special mental function as well as in general is slightly transferable to other functions and that the greater the fatigue the greater the transferred fatigue”; and (4) “as a rule, the more competent people are less affected by fatigue.” Martyn (13) tested three individuals as to (a) spatial threshold, (b) muscular capacity, (c) respiration, (d) pulse, and (e) speed and accuracy of perception before and after an hour of multiplication. She found no significant indication of fatigue in the five tests nor in the work of multiplication.

Three other investigations have been made concerning the nature of mental fatigue, but related specifically to school problems. Winch¹ has already shown that children improve in arithmetic more rapidly when the class meets in the morning than in the afternoon. He now adds to this information concerning fatigue-effects from school work at different hours of the day two further facts. (1) There is a reduction in efficiency of about $5\frac{1}{2}$ per cent. in one group and 2 per cent. in another group in the late afternoon as compared with the early morning when measured by immediate memory tests (26). And (2) children in classes meeting early in the morning do not

¹ W. H. WINCH. Mental Fatigue in Day-School Children, as Measured by Arithmetical Reasoning. *Brit. J. of Psychol.*, 1911, 4, 315-341.

improve so rapidly in arithmetic as those who are in classes meeting later in the morning. This was true of all but one school,—a school of poor boys who rose early and worked before going to school. This group made the greatest improvement in the early morning class (25). Robinson (18) confirms Winch in his conclusion that there is a noticeable warming-up effect at the beginning of the morning session,—most noticeable in older children. But this warming-up is only a phase of a large variation in efficiency for different hours of the day,—a variation which shows itself in a rise in efficiency to about 10.30 A. M., in a drop to 12.30 P. M. and a rise again till the close of school at 2.00 P. M. Robinson also confirms previous work in finding that a recess of 10 to 15 minutes is of decided value but that longer periods cause a loss of efficiency in the work immediately following. The same was found true regarding the length of periods devoted to gymnastics and singing.

Lagrange (9) reviews the subject of fatigue from the physician's standpoint and discusses the proper use of exercise and rest in the cure of fatigued patients.

A year ago Wells¹ reviewed the first two sections of Bogardus's work. In the third section (2) we have presented: (1) facts which go to show that "there is a rise in the number of accidents during the forenoon, a decided fall after the noon period of rest, and another rise in the afternoon hours." Also, that with the exception of Monday there is a steady increase in the number of accidents as the week progresses. But on Monday there are more accidents than on the next three following days. Evidently, Sunday recreation unfits workmen for steady work on Monday. (2) Various judicial decisions respecting fatigue. (3) A general conclusion: "Uninterrupted work is accompanied by muscular inaccuracy which increases irregularly and at a rate dependent on the rate of activity and on the relative difficulty of the given work for the given individual." In other words, "Fatigue is a cause of industrial accidents." (4) A list of methods to prevent fatigue, hence accidents.

Monotony is generally viewed as a cause of fatigue. Münsterberg (14), however, holds another view. From observation and experiment he concludes that "there are persons who after they have received an impression are unable immediately to seize the same impression again. . . . But there are evidently other persons for whom, on the contrary, the experience of an impression is a kind of inner preparation for arousing the same or similar impressions."

¹ F. L. WELLS. *Fatigue*. *PSYCHOL. BULL.*, 1912, 9, 416-420.

The former group rebel against the monotony of a job, because it is an effort to seize each succeeding presentation, while the latter group, having no such difficulty, do not find fault with their work. He urges the use of experimental tests as an aid in the vocational guidance toward monotonous or non-monotonous tasks. Münsterberg also devotes some attention to the factor of noise as related to fatigue. Noises, such as conversation, and those of a rhythmical nature, can very often be prevented.

Lehmann (10) raises the question as to the relationship between mental work and the waste-products of the body. From various experiments he concludes that "mental work of a determined kind and amount has with the same individual a constant increase in the amount of exhaled CO_2 and corresponds to a constant energy measure just as does physical work." Also, that the production of CO_2 will be throughout so much the greater, the greater is the difficulty of the mental work as measured by the exertion of the attention. According to Hellpach (8), who reviewed the discussion that followed Lehmann's paper, Exner contended that there were many involuntary muscular movements, as well as organic changes, which Lehmann could not take into account and states that only an examination of the waste-products of the brain itself can be used to settle the question. Hellpach feels that this is not necessary, as the organism is a unit, so that no organ can act independently of the whole organism. Hence, a measure of brain activity must include not only its own activity but that of all the organs which are stimulated by its activity.

Dodge (4) pleads for a measure of mental work in terms of some organic change. If work is measured in terms of time or of amount of performance, many factors are not taken into account which undoubtedly play a rôle in the total situation. Previous investigations have shown that muscle-work may be measured in terms of metabolism and expressed in thermo-dynamic units through calorimetric determinations; also, that changes in pulse rate are closely correlated with changes in metabolism. Dodge has by means of a very ingenious technique found it possible to correlate various types of physical and mental work with the pulse-rate, and in this way to determine that the mental work of multiplication, for example, is closely approximated by the raising once every four seconds of a one-pound weight from the relaxed position of the arm hanging vertically to the horizontal position of the forearm. Urban (22) in reply contends that, even if all experimental difficulties can be

removed from such determinations, we shall not even then have a measure of mental work, but only a measure of the physiological changes correlated with the psychical processes.

Lorentz (11) reports that the use of Weichardt's antikenotoxin actually resulted in an improvement in both muscular and mental work. He suggests that its use may be of great advantage in the school-room at the end of the day. The writer is not, however, personally convinced that the experiments were conducted with sufficient care to be conclusive.

Starch (20) had four groups of subjects employed in a substitution test for a total of 120 minutes each. The first group worked 10 minutes at a time, twice a day for 6 days, the second group 20 minutes at a time once a day, the third group 40 minutes on alternate days, and the fourth group worked 120 minutes at a stretch. The first group accomplished the most, with the second group a close second. Both accomplished considerably more than the other two groups. Pyle (17) objects to this work on the ground that the two factors "length of working period" and "interval between them" were varied at the same time. From using another form of the substitution test he concludes: (1) that a working period of 30 minutes a day is more favorable for improvement per time consumed than are periods of 15, 45, or 60 minutes a day; and (2) that *daily* work is superior to either *alternate day* work or to *twice a day* work. The second practice on the same day is not quite so beneficial as the first practice in the early stages of learning but in the later stages the second practice becomes useless.

Wells raises a number of interesting problems regarding practice and fatigue in continued work,—problems of great interest to those who are attempting vocational guidance on the basis of psychological tests. His work-periods lasted, however, but five minutes. In the first article (23) he raises the problem as to whether a high initial efficiency is the result of (1) greater previous practice or (2) greater ability to profit by a given amount of practice. If the first is true we should expect that further practice will bring about a relatively smaller amount of improvement than shown by one who had a low initial performance; if the second is true, the reverse should be the case. He concludes from his experimental results that "the ability to improve" is a more important influence than "the amount of practice" in determining the amount of gain which may accrue from practice. In another study (24) of the same experimental data, Wells points out that the general effect of practice upon endurance

periods in addition is a favorable one. "That is, the subject does not lose so much by fatigue in the later stages of practice as in the earlier ones." But in the number-checking test there is no such effect.

Forbes (7), following Sherrington, studied the cause of rhythmic responses which follow the combined effects of excitatory and inhibitory stimuli. He suggests that the situation is analogous to a stream of air passing out from a tube under water. It is a fact that the deeper the mouth of the tube the larger and more infrequent are the air-bubbles. In the same way we find that the greater the inhibition, the more intense but less frequent are the responses from the double source of excitation. The explanation is, of course, that the inhibitory force (*A*) opposes the excitatory force (*B*) and tends to keep it pent up or potential. When *B* finally becomes greater than *A* it discharges and in doing so it proceeds until more energy is released than is represented by the excess of *B* over *A*. In this way we obtain the rhythmic condition of discharge and quiescence.

Using a plethysmograph Patrizi (16) studied the volumetric changes in the arm which accompanied the presentation of the signal "Now," followed at irregular intervals by the ringing of an electric bell. Alternately, the subject was expected (1) to offer no resistance to the vasomotor changes and (2) to inhibit them. Normal male adults were found able to inhibit such changes more than females, while both were more able in this respect than children or alcoholics. In another study (15) Patrizi obtained fatigue curves from two ergographs used simultaneously, one with each hand. During the experiment the subject was required to perform some mental task. It was found that subjects fell into two groups,—the "dynamogènes" and the "inhibiteurs." The ergographic work with the former was increased during the mental test, while with the latter the work was decreased as compared with what was normally to be expected. These two groups seemed to be constant when tested over a considerable period of time.

Culler's (3) work was reviewed by Baird in the September number of the *BULLETIN* and need only be included in the list of references here. The discussions by Sherrington, (19), Tait (21), and Lucas (12) are reported by title only.

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REACTION TIME

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The reaction time literature of the year consists of two doctorate dissertations and various minor papers dealing chiefly with apparatus and technique.

Poffenberger (10) attempts to isolate the synapse-time or the time lost in conduction in nerve centers. His experiments consist of an intensive investigation of reaction time to retinal stimulation at the fovea and of points 3, 10, 30, and 45 degrees from the fovea in a horizontal plane. He finds that the length of reaction times on eccentric positions is always greater than on the foveal position; that the length of the time increases as the distance from the fovea is increased; and that the reaction time to temporal stimulation is longer than to nasal stimulation. Time relations between the temporal and the nasal sides of the retina agree with the extents of the corresponding fields, with acuity relations, and with the appearance of flicker. There is a slight but significant difference in reaction time of the right and left hands, a difference following stimulation of one eye or both eyes, but no difference in reaction time with hand and eye on the same side and on opposite sides of the body. The differences between the reaction times on the direct path and on the indirect path for the two most highly practiced subjects were 3.6σ and 4.0σ. This is regarded as the time lost in conduction in nerve centers or the synapse-time in man.

Todd (11), using light, electric shock, and sound stimuli, finds that the reaction time to two simultaneous stimuli is less than to either of them alone; to a group of three simultaneous stimuli is less than to any single component member or to any pair of the group; and that when another stimulus is added to a simple stimulus or to two stimuli there is a reduction in the reaction time, the amount of the reduction being dependent upon the reaction time to the stimulus added. There seems to be no evidence that a reagent is able to select from a pair or group of simultaneous stimuli one of its members for reaction. When stimuli of medium intensities are presented with others of low intensities, there is a decrease in the reaction time. The reaction time to a light stimulus preceded by a sound and electric shock stimulus at regular intervals is longer than the reaction time to light alone.

Henri and Languier des Bancelles (5, 6) contrast the reaction times in higher animals, in which the time of cerebral processes is long and the latent time in the sense-organs short, with that in lower organisms (Cyclops stimulated by ultraviolet light) in which the time required for excitation of the sense-organs is very great in relation to the total reaction time. Thus, for a reaction time with Cyclops of 500 σ , the sensory excitation requires at least 400 σ .

Langier and Richet (9), using reaction time as a test of fatigue in a stenographer after from five to twelve hours of work, find a very marked increase in times and their variability. Twenty tests after five hours showed an increase in times of 49 σ and 80 σ , after nine to twelve hours 27 σ , and after seven and a half hour of work with two hours of rest at noon 72 σ .

Deuchler's (1) previous paper gave a detailed account of reactions for the individual subjects. The continuation deals at great length with common factors in (1) reactions with indefinite expectation, such as reduction in times with practice and decrease in variability, the influence of the practice series on later reactions, various modes of adjustment in experiments, variations with different senses, the effects of the foresignal, etc., (2) reactions with definite expectation with a regular change in direction of attention from experiment to experiment; (3) the influence of variation in foresignals; (4) reactions to three simultaneous stimuli with a regular change of attention to individual members of the complex. Contrary to Todd's findings, Deuchler reports that reactions to three stimuli varied according to which was apperceived.

Westphal (12) studies the changes in pulse and breathing in choice reactions to two auditory stimuli (reaction with the second finger of the right hand to the sound of a hammer and with the third finger to the sound of a bell). He compares expressive symptoms during the experiment with those under normal conditions and also the variations during the various stages into which the experiment can be divided: (1) the normal state; (2) the period from the ready signal to the foresignal; (3) the period from the foresignal to the giving of the stimulus; (4) the period from the reaction to the first breath after reaction; (5) the period from the second to the fourth breath after reaction. Among other things Westphal finds that the changes in pulse and breathing with the direction of feeling in volitional processes point to the fact that the feeling of activity is composed of partial feelings of tension and excitement.

Dunlap (2, 3) describes an apparatus for measuring association

reactions, consisting chiefly of two voice keys, an Ewald chronoscope, a master switch, and a fork of 64 d. v. with two mercury contacts. Evans (4) describes a compact and convenient arrangement of the pendulum chronoscope and accessories for reactions to light, sound, and touch. Kiesow (7, 8) describes in detail, with accompanying figures, two æsthesiometers, one for warmth and the other for contact. The æsthesiometer for reaction times to warmth consists of an electrically heated platinum coil which permits of the isolation of warmth stimuli.

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SPECIAL REVIEWS AND REPORTS

Bewusstsein und psychisches Geschehen. L. LOEWENFELD. Wiesbaden, 1913. Pp. vi+94.

The author holds that the realm of the psychic is far broader than that of consciousness. Psychophysical parallelism and inference from behavior both force us to assume the existence of a psychic subconscious. After the usual citation of facts from normal and abnormal psychology which support this hypothesis comes an inquiry into the nature of this "night side of the soul." We must not conceive it as a second ego, or identify it with the activities shown during hypnosis and in secondary personalities, for these show the same separation between conscious and subconscious phenomena that is evident in normal states. Rather we must think of it as containing the same sorts of processes that are, under other conditions, material for consciousness. The subconscious underlies and supports consciousness, it is the source of many moods, feelings, sympathies, and aversions. It varies in its range with the individual and with the period of his development. In general, its relation to consciousness is that of the subordinate to whom the working out of details is entrusted. Consciousness sets up the goals of our thought, takes general charge and oversight of new operations. Old ways of thinking, paths of least resistance, are likely to be subconscious. The facts of memory, posthypnotic suggestion, and the like, are then treated at some length in the effort to demonstrate that subconscious processes are involved.

In conclusion, the author pleads for a more scientific attitude on the part of all concerned toward the problem in hand. The Freudians have given us material in this field that will be valuable when their conclusions are sifted, but not until then. The orthodox psychologist, on the other hand, has been blind to the nuggets of truth which the psychoanalyst has unearthed. Unprejudiced investigation is the crying need.

The book, while summarizing well the present status of the question of the subconscious, contains little that is new. On the whole, it is a vivid reminder that investigations in this field have not yet passed out of the anecdotal stage. In these days of increasing

emphasis on the study of behavior, it seems unfortunate that so much stress should be laid on attempts to prove the psychic nature of "subconscious" activities. We need rather attempts to bring the phenomena in question under experimental control, to describe and classify them, before we set up hypotheses as to their ultimate nature.

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PSYCHOLOGY AT THE RECENT MEETING OF THE
BRITISH ASSOCIATION FOR THE ADVANCEMENT
OF SCIENCE (BIRMINGHAM, SEPTEMBER
10-17, 1913)

This year, for the first time, a subsection for psychology was established by the British Association. Hitherto psychological papers have been heard chiefly in the section of Physiology, which is still sponsor for the newly formed subsection. Judging by the large attendance at the meetings, the considerable number of papers, and the interest aroused, it is highly probable that the new division will soon demonstrate its claims to independence.

The meetings occupied five days, and were ably presided over by the chairman of the subcommittee, Prof. J. H. Muirhead, while Mr. Cyril Burt acted as recorder. Plans were laid to continue the subsection at the next meeting, which is to be held in Australia, and it is confidently expected that the 1915 meeting at Manchester will be even more successful than the past occasion has been. Two joint meetings were held, one with the section of Physiology and one with the section of Education.

Some thirty-five papers, covering a wide variety of topics, were read. Of these applied psychology had the chief share, with some nineteen titles, of which twelve were in educational psychology.

Among the papers which dealt with general and systematic problems may be mentioned the following: (1) H. Wildon Carr, *The Absurdity of Psycho-physiological Parallelism even as an Hypothesis*. The speaker's thesis was that since consciousness is not of the nervous processes with which it is correlated, but of the whole physical world which brings about the nervous activities, therefore it is absurd to assume that the part (or cortical processes) is equivalent to the whole physical universe of which consciousness makes us aware. (2) William McDougall, *A New Theory of Laughter*.

Laughter is a protective reaction, occasioned by minor distresses which would have a cumulative depressing influence were it not for the acquired capacity to drain off the nervous energy thus aroused through the channels of the laughter reaction. The typical expression of pleasure is not laughter but the smile, which is of a different order and appears earlier in childhood. (3) H. J. Watt, *Some Main Principles of Integration*. This paper dealt with the possibility of proceeding on the basis of sensation through integrative stages to the higher mental processes. (4) Carveth Read, *The Condition of Belief in Immature Minds (Children and "Savages")*. The main point of this discussion was that all beliefs have their fundamental ground in perception. The imaginative beliefs arise largely in minds which lack a definite logical test for truth, but it is wrong to suppose that they are as profound as those based upon perception. Their utility has kept them alive, but there is no justification in tracing scientific ideas to them. (5) G. J. Stokes, in his paper on *The Relation of the Emotions to Motor Discharge*, advanced the theory that the nervous correlate of emotion might be sought in the nervous processes which connect the sensory and motor centers of the cortex.

In the field of experimental psychology, C. S. Myers reported on *Experiments in Sound Localization* now being conducted in the very carefully constructed sound-proof room of the Cambridge laboratory. Results seem to indicate that the localization of sound in the median (sagittal) plane are, at best, very inaccurate, and that learning to localize is primarily dependent upon loudness and timbre. Tactual impressions felt by the subject upon the face and head appear upon analysis to be illusory. R. M. Ogden reported upon *The Spatial Localization of Visual Images*. The complete report is published in the September issue of the *Psychological Review*. Miss Mary Smith read a paper on *Two Forms of Memory and their Relation*, which reported a series of experiments undertaken to test the validity of Bergson's distinction of the two types of memory. Five tests were used. Two dealt with mechanical associations in nonsense syllables and motor activities, while three involved recognition, analytic ability and intelligence. A high degree of correlation was established between the tests of each group, while the correlation was low between tests of the two groups.

"Imageless thought" came in for a share of the discussion in the paper of Miss S. S. Fairhurst (read by C. S. Myers) on the *Analysis of the Mental Processes Involved in Spelling*. Her investigation indicated that "imageless" spelling is an important factor, which

involves a distinction between the knowledge of the facts of spelling and mere reference to imagery. Also in the paper on the *Conditions which Arouse Mental Imagery in Thought*, C. Fox reported experiments in which the meaning-process aroused by a series of statements, true and false, was introspected. Mental images were found to be absent in about 50 per cent. of the reports, and when present they seemed to be occasioned by some delay or hindrance in the process. It was of some interest to note that the acceptance of imageless contents appeared to arouse no serious opposition.

The Freudian psychology was debated in connection with a paper on *Psycho-Analysis* by William Brown, in which the Freudian theory was summarized, and in *An Analysis of Some Personal Dreams with Special Reference to Current Theories of Dream Interpretation*, by T. H. Pear. The last-named speaker discussed dreams illustrative of the dramatizations, condensations and displacements which the Freudian views have made familiar. A second paper by T. H. Pear and one by Stanley Wyatt dealt with the psychology of testimony in normal and defective children.

Among the papers which bore upon educational psychology may be mentioned Miss I. Suddard's *Investigation into Spelling in the Fielden Demonstration School*, which indicated the appearance of the "spelling disease" with the beginning of free composition, and the possibility of lessening its evil effects by postponing free composition and keeping the work under the teacher's control until proper spelling habits had been formed. Miss S. S. Fairhurst, whose work, mentioned above, formed the basis for two reports, found that the articulation of letters was of no direct aid in spelling, but that the articulation of syllables, which constitute the phonic units of spelling, when simultaneous with the writing of the word, is probably the best method of learning to spell. Her results indicated further that spelling efficiency is independent of the imaginal type of the speller.

Sir William Ramsay found the occasion of these reports emphasizing the difficulties of learning to spell opportune for a few remarks in support of the English movement to reform spelling by making it phonetic.

C. W. Valentine reported some experiments on the *Method of Teaching Reading*, which indicated that the phonic method is preferable to the "look and say" method.

E. O. Lewis reported some experiments in learning of unfamiliar words and pictures, which substantiated previous indications that the

method of learning by wholes is better than that of learning by parts, although in the case of unfamiliar words the syllabic unit was found to be economical.

A mass investigation on *Practice Improvement in Immediate Memory in School Children* was reported by J. L. McIntyre. The tests were made with meaningless syllables, and the results showed the marked superiority of girls over boys, and of town over country children. It was also shown that a considerable amount of improvement of ability gained by memorizing syllables was available for ordinary school work.

Miss A. L. Rogers reported upon the *Application of the Binet-Simon Tests to Normal Children in Scotland*, which was made in conjunction with Dr. McIntyre. The scale was found to be too easy for the early years and too hard for the later years, but on the whole reliable. More exceptional boys, both gifted and deficient, were found than girls.

R. C. Moore's paper on *Tests of Reasoning and their Relation to General Ability* (read by C. Burt) and W. H. Winch's paper on *Additional Tests for Reasoning Suitable for the Mental Diagnosis of School Children* indicated the feasibility of such tests. Mr. Moore's analogies, syllogisms and arguments showed a higher correlation with teachers' estimates than do the average Binet tests, while Mr. Winch's ingenious problems seemed to furnish a natural means of testing reasoning ability unrelated to school work. Mr. Cyril Burt's mass investigation on the *Mental Differences between the Sexes* indicated that the differences grow progressively slighter as one proceeds from instinctive to acquired capacities. The females showed superiority in skin discrimination, memory, reading, writing and finding opposites; the males in tapping tests, addition, multiplication, argument and syllogistic exercises.

Space permits but brief mention of the remaining papers which were presented. C. W. Valentine reported an ingenious test of *Color Perception and Preference in a Three Months Old Infant*. Nine colored wools were presented to the child in pairs for two minute intervals and the time in which each color was attended to was recorded. The results indicated the following order of preference: (1) yellow, (2) white and pink, (3) red, (4) brown and black, (5) green and blue, (6) violet. The brightest colors were in general preferred but the marked difference in preference between red, green, blue and violet, all of which were equally bright, is noteworthy.

Godfrey Thompson's paper on *Variations in the Spatial Threshold*

reported upon experiments which indicated a sharp fall in the threshold during the first twenty judgments, followed by a slow rise to about forty, after which it became erratic, but often fell again at the end of the sitting. The speaker concluded that the average threshold is a matter of mathematical definition and not a psychological experience: that for psychology no real threshold can be established.

Only one paper was read on animal psychology, *A Note on Habit-Formation in Guinea Pigs* by Miss E. M. Smith, in which the speaker described two tests now being used in the Cambridge laboratory with the aim of studying the inheritability of such characters as learning, practice, accuracy, retentiveness, etc.

Shepherd Dawson's *Simple Method of Demonstrating Weber's Law* was the only title which suggested the technique of apparatus. Mr. Dawson's method consists in rotating white and gray disks punctured upon a radius with equally sized holes against a black background. The relative brightness of the rings thus produced tests the law by the simple procedure of counting the number of rings which can be seen on each disk.

A mass investigation on *The Relative Fertility and Morbidity of Normal and Defective Stocks* showed greater fertility and morbidity for the defective than for the normal stocks.

Two papers bearing upon fatigue were read, by Miss Mary Smith: *Some Experiments on Recovery from Fatigue*, and by J. H. Wimmis: *A Comparative Investigation of Fatigue Tests*.

Dr. Auden read the report of the Committee of the Education section on *The Relation of School Books to Eyesight*. The Rev. J. Knowles addressed himself to *The Need of a Common Alphabet for the Vernacular Languages of India*. *A Plea for Research in Education* was made by C. W. Kimmins and supported by brief remarks from C. S. Myers, J. J. Findlay, J. A. Green and C. Burt. A paper on *The Excessive Use of Suggestion in Education* was presented by Mrs. Meredith, while the concluding title of the program was *Contrast as a Factor in Psychological Explanation*, by W. G. Smith.

R. M. OGDEN

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